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THE PRINCIPLES OF MECHANICS WITH NEWTON FROM 1679 TO 1687.*

I.

MANY times on the subject of Newton's optical publications, Hooke had claimed priority, and there was in consequence some friction between Hooke and Newton. That such disputes were agreeable neither to Hooke nor to Newton appears from letters which passed between them in 1676.[1]

It is probable that the intermeddling friend, referred to by Hooke, who stirred up strife between them by painting Hooke in dark colors to Newton was Henry Oldenburg, the secretary of the Royal Society.[2] In the disputes between Hooke and Hevelius on the subject of plain and telescopic sights, and between Hooke and Huygens about the invention of pendulum clocks, Oldenburg took a keen and active part against Hooke. However, in a later dispute of 1686, and therefore long after Oldenburg's death, Halley remarked to Newton that he feared that Hooke's manner of claiming the discovery of the law of the inverse squares had been "represented in worse colors than it ought."[3]

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* The references are mainly to the works mentioned in my previous article in this journal (April, 1914) on "The Principles of Mechanics with Newton up to 1679."

[1] Brewster, *op, cit.,* pp. 140-143.

[2] *Ibid.,* pp. 143-144.

[3] Rouse Ball, *op. cit.,* p. 163; Brewster, *op cit.,* p. 144.

We shall now have to consider some correspondence that passed between Hooke and Newton in 1679 and 1680, which is very important, since it was the means of turning Newton's thoughts again to the subject of gravitation. It also forms the ground-work of the claim put forward later by Hooke to the discovery of the Newtonian theory of gravitation.

The "Royal Society for the improving of Natural Knowledge" was founded in 1662.[4] As mentioned in my article on "Robert Hooke as a Precursor of Newton,"[5] Hooke was its first "curator of experiments" and Henry Oldenburg its first secretary. When[6] Oldenburg died in 1677 Hooke succeeded him as secretary, and wrote to Newton on November 24, 1679, as follows:[7]

"Sir,

Finding by our registers that you were pledged to correspond with Mr. Oldenburg, and having also had the happiness of receiving some letters from you myself, make me presume to trouble you with this present scribble—Dr. Grew's more urgent occasions having made him decline the holding correspondence. And the Society has devolved it on me. I hope, therefore, that you will please to continue your former favors to the Society by communicating what shall occur to you that is philosophical, and, for return, I shall be sure to acquaint you with what we shall receive considerable from other parts, or find out new here. And you may be assured that whatever shall be so communicated shall be no otherwise further imparted or disposed of than you yourself shall prescribe. I am not ignorant that both heretofore, and not long since also, there have been some who have endeavored to misrepresent me to you, and possibly they or others have not been wanting

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[4] It celebrated its 250th anniversary in July, 1912.

[5] *Monist*, July, 1913, § 1.

[6] Cf. *ibid.*, § VII.

[7] Rouse Ball, *op. cit.*, pp. 139-140.

[lacking] to do the like to me; but difference in opinion, if such there be, should not, especially in philosophical matters where interest hath little concern, methinks, be the occasion of enmity—it is not with me, I am sure. For my part I shall take it as a great favor if you shall please to communicate by letter your objections against any hypothesis or opinion of mine; and particularly if you will let me know your thoughts of that of compounding the celestial motions of the planets out of a direct motion by the tangent and an attractive motion towards the central body, or what objections you have against my hypothesis of the laws or causes of springiness."

Thus began the first letter of a correspondence[8] through which Newton was, as Pemberton says, led to inquire what was the real figure in which a body, let fall from any high place, descends, taking the motion of the earth round its axis into consideration; and, as Newton acknowledged in a letter[9] written in 1686 to Halley:

"This is true, that his [Hooke's] letters occasioned my finding the method of determining figures, which, when I had tried in the ellipsis, I threw the calculations by, being upon other studies; and so it rested for about five years, till upon your request I sought for that paper; and not finding it, did it again, and reduced it into the propositions shown you by Mr. Paget: but, for the duplicate proportion,

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[8] Brewster, *op. cit.,* Vol. I, pp. 291-292, 254-255; Rouse Ball, *op. cit.,* pp. 18-24, 139-150, 155-167. It is very likely that Newton had his attention first directed to Picard's measurement by Hooke (see below), and Brewster (*op. cit.,* pp. 291-292; cf. Rouse Ball, *op. cit.,* pp. 22-24) seems wrong in stating that Newton did not resume his former thoughts concerning the moon till 1684 (cf. Pemberton in Rouse Ball, *op. cit.,* p. 20). It is amusing to compare Conduitt's (Brewster, *op. cit.,* pp. 291-292) version of the correspondence with Hooke with the actual letters; for Conduitt stated that he got his information from Newton. Newton, in his first letter to Hooke, explicitly said "spiral line"; Conduitt attributed the error to a "negligent stroke of the pen" in the diagram. Newton himself wrote to Halley on May 27, 1686: "In the scheme wherein I explained this, I carelessly described the descent of the falling body in a spiral to the center of the earth: which is true in a resisting medium, such as our air is." Cox (*op. cit.,* p. 93) is mistaken in saying that Newton worked out the details of his discovery from 1672 (when Picard communicated his results to the Royal Society) to 1674.

[9] Rouse Ball, *op. cit.,* p. 165.

I can affirm that I gathered it from Kepler's theorem about twenty years ago."

Again, in a letter written to Halley thirteen days afterwards, Newton said:[10]

"And, though his correcting my spiral occasioned my finding the theorem by which I afterwards examined the ellipsis, yet am I not beholden to him for any light into the business, but only for the diversion he gave me from my other studies to think on these things, and for his dogmaticalness in writing as if he had found the motion in the ellipsis, which inclined me to try it, after I saw by what method it was to be done."

For this reason we will here consider the correspondence in some detail.

In the first letter Hooke, after the opening paragraph quoted above, mentioned two things. Firstly, he desired to know Newton's opinion of a hypothesis of the heavens, invented by Mallemont de Messanges;[11] and, secondly he mentioned that Picard and others were making geodetical observations in France.[12] "I am informed, likewise from Paris, that they are there about another work, viz., of settling the longitude and latitude of the most considerable places: the former of those by the eclipses of the satellites of Jupiter. MM. Picart and De la Hire travel, and MM. Cassini and Romer observe at Paris. They have already found that Brest in Brittany is eighteen leagues nearer Paris than all the maps make it. I have written to a correspondent in Devonshire to see if he can do somewhat of that kind here, and I should be glad if, by perpendicular observations, we could determine the difference of latitude between London and Cambridge. If you know of any one that will observe at Cambridge, I will procure it to be done here very exactly."

[10] *Ibid.*, 167.

[11] On this man, see Rigaud, *op. cit.*, pp. 42-43.

[12] Rouse Ball, *op. cit.*, p. 140.

Hooke finally gave some small items of news about a book by De la Hire and some observations of Flamsteed's.

This letter was copied by Rouse Ball from a rough draft in Hooke's handwriting which is in the Library of Trinity College, Cambridge;[13] and it must be remembered when reading Rigaud's *Essay,*[14] that Rigaud had not seen this correspondence.

It was probably this letter which led Newton to use Picard's estimate in the revision of his calculation.[15]

Newton replied on Nov. 28, 1679,[16] and his letter began:

"I cannot but acknowledge myself every way by the kindness of your letter tempted to concur with your desires in a philosophical correspondence. And heartily sorry I am that I am at present unfurnished with matter answerable to your expectations—for I have been this last half year in Lincolnshire cumbered with concerns amongst my relations till yesterday when I returned hither; so that I have had no time to entertain philosophical meditations, or so much as to study or mind anything else but country affairs. And before that, I had for some years last been endeavoring to bend myself from philosophy to other studies, in so much that I have long grudged the time spent in that study unless it be perhaps at idle hours sometimes for a diversion; which makes me almost wholly unacquainted with what philosophers at London or abroad have of late been employed about. And perhaps you will incline the more to believe me when I tell you that I did not, before the receipt of your last letter, so much as hear (that I remember) of your hypothesis of compounding the celestial motions of the planets out of a direct motion by the tangent to the curve, and of the laws and causes of springiness, though these no doubt are well known to the philosophical world. And having thus shook hands with philosophy and being also at present taken up with other business, I hope it will not be interpreted out of any unkindness to you or the Royal Society that I am backward in engaging myself in these matters, though formerly I must acknowledge I was moved by other reasons to decline, as much as Mr. Oldenburg's importunity and ways to engage me in disputes would permit, all correspondence with him about them. However I cannot but return my hearty thanks for your thinking me worthy of so noble a commerce and, in order thereto, frankly imparting to me several things in your letter."

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[13] *Ibid.,* p. 139.

[14] Cf. pp. 12, 37, 42.

[15] Rouse Ball, *op. cit.,* p. 23; cf. the quotation of Pemberton's confirmatory evidence and Robison's—probably mistaken—account on pp. 20-23. Cf. Brewster, *op. cit.,* 292.

[16] Rouse Ball, *op. ct.,* pp. 141-144.

In a letter of June 20, 1686, to Halley, Newton thus[17] described this correspondence:

"....In my answer to his first letter I refused his correspondence, told him I had laid philosophy aside, sent him only the experiment of projectiles (rather shortly hinted than carefully described), in compliment to sweeten my answer, expected to hear no further from him; could scarce persuade myself to answer his second letter; did not answer his third,—was upon other things; thought no further of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time."

Indeed, Newton, after briefly discussing Mallemont's hypothesis, and after referring to a possible determination of latitude and to Flamsteed's observations,[18] proceeded[19]:

"In requital of this advertisement, I shall communicate to you a fancy of my own about discovering the earth's diurnal motion. In order thereto, I will consider the earth's diurnal motion alone, without the annual, that hav-

[17] *Ibid.*, p. 157.
[18] *Ibid.*, p. 142.
[19] *Ibid.*, pp. 142-143.

ing little influence on the experiment I shall here propound. Suppose then BDG represents the globe of the earth carried round once a day about its center C from west to east, according to the order of the letters BDG; and let A be a heavy body suspended in the air, and moving round with the earth so as perpetually to hang over the same point thereof, B. Then imagine this body A let fall; its gravity will give it a new motion towards the center of the earth without diminishing the old one from west to east. Whence the motion of this body from west to east, by reason that before it fell it was more distant from the center of the earth than the parts of the earth at which it arrives in its fall, will be greater than the motion from west to east of the parts of the earth at which the body arrives in its fall; and therefore the body will not descend the perpendicular AC, but, outrunning the parts of the earth, will shoot forward to the east side of the perpendicular, describing in its fall a spiral line ADEC, quite contrary to the opinion of the vulgar who think that, if the earth moved, heavy bodies in falling would be outrun by its parts and fall on the west side of the perpendicular. The advance of the body from the perpendicular eastward will, in a descent of but twenty or thirty yards, be very small, and and yet I am apt to think it may be enough to determine the matter of fact. Suppose, then, in a very calm day, a pistol bullet were let down by a silk line from the top of a high building or well, the line going through a small hole made in a plate of brass or tin fastened to the top of a building or well; and that the bullet when let down almost to the bottom were settled in water so as to cease from swinging, and then let down further on an edge of steel lying north and south to try if the bullet in settling thereon will almost stand *in aequilibrio* but yet with some small propensity (the smaller the better) decline to the west side of the steel as often as it is so let down thereon. The steel being so placed underneath, suppose the bullet be then drawn up to the top and let fall by cutting, clipping, or burning the line of silk. If it fall constantly on the east side of the steel, it will argue the diurnal motion of the earth. But what the event will be I know not, having never attempted to try it. If anybody would think this worth their trial, the best way, in my opinion, would be to try it in a high church or wide steeple, the windows being first well stopped; for, in a narrow well, the bullet possibly may be apt to receive a ply from the straitened air near the sides of the well, if in its fall it come nearer to one side than to another. It would be convenient also that the water into which the bullet falls be a yard or two deep or more, partly that the bullet may fall more gently on the steel, partly that the motion which it has from west to east at its entry into the water may, by means of the longer time of descent through the water, carry it on further eastward and so make the experiment more manifest."

In the figure which accompanied this letter, the path of the falling body from A to D was indicated by a dotted line which was continued into the interior of the earth, and, after having turned once round the center C, ended at C. This is the spiral often referred to below, as well as by Newton in a note above.

This letter was read to the Royal Society on Dec. 4, 1679, as appears by the following minute:[20]

"Mr. Hooke produced and read a letter of Mr. Newton to himself dated 28th November, 1679, containing his sentiments of Mallemont's new hypothesis of the heavens; and also suggesting an experiment whereby to try whether the earth moves with a diurnal motion or not, viz., by the falling of a body from a considerable height, which, he alleged, must fall to the eastward of the perpendicular, if the earth moved. This proposal of Mr. Newton's was highly approved of by the Society; and it was desired that it might be tried as soon as could be with convenience. Sir Christopher Wren supposed that there might be something of this kind tried by shooting a bullet upwards at a certain angle from the perpendicular round every way, thereby to see whether the bullets so shot would all fall in a perfect circle round the place where the barrel was placed. This barrel he desired might be fixed in a frame upon a plane foot, and that foot placed upon a true plane every way, and the mouth of the gun be almost in the same point over the plane which way soever shot. Mr. Flamsteed hereupon alleged that it was an observation of the gunners that, to make a ball fall back into the mouth of the piece, it must be shot at eighty-seven degrees; and that he knew the reason thereof; and that it agreed with his theory; and that a ball shot perpendicularly would never fall perpendicularly; and he mentioned the recoiling of a perpendicular jet of water. But this was conceived to arise from some mistake of the gunners in not well taking notice of all circumstances; since a body shot perpendicularly would also descend perpendicularly; and a body shot at eighty-seven degrees would fall considerably distant from the place where it was shot."

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[20] Birch, *op. cit.*, Vol. III, 1757, pp. 512-513; Rouse Ball, *op. cit.*, p. 145.

Newton's letter then ended:[21]

"If I were not so unhappy as to be unacquainted with your hypothesis above mentioned (as I am with almost all things which have of late been done or attempted in philosophy), I should so far comply with your desire as to send you what objections I could think of against them, if I could think of any. And, on the other hand, I could with pleasure hear and answer any objections made against any notions of mine in a transient discourse for a divertisement. But yet, my affection to philosophy being worn out, so that I am almost as little concerned about it as one tradesman uses to be about another man's trade or a country man about learning, I must acknowledge myself averse from spending that time in writing about it which I think I can spend otherwise more to my own content and the good of others; and I hope neither you nor anybody else will blame me for this averseness. To let you see that it is not out of any shyness, reservedness, or distrust that I have of late and still do decline philosophical commerce, but only out of my applying myself to other things, I have communicated to you the notion above set down (such as it is) concerning the descent of heavy bodies for proving the motion of the earth; and shall be as ready to communicate in oral discourse anything I know, if it shall ever be my happiness to have familiar converse frequently with you. And possibly if anything useful to mankind occurs to me I may sometimes impart it to you by letter."

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[21] Rouse Ball, *op. cit.*, p. 144.

Hooke replied on Dec. 9, 1679, by a letter which has been lost. A note, however, written by Hooke, contains the words:[22]

"Dec. 9th, 1679, in a letter to Newton, Hooke has these words upon an account of an experiment about the falling of a ball from a considerable height: I could add many other considerations consonant to my theory of circular motions compounded by a direct motion and an attractive one to the center, etc."

It was read to the Royal Society on Dec. 11, 1679:[23]

"Upon the mentioning of Mr. Newton's letter and the experiment proposed in it, Mr. Hooke read his answer to him upon that subject, wherein he explained what the line described by a falling body must be supposed to be, moved circularly by the diurnal motion of the earth and perpendicularly by the power of gravity: and he showed that it would not be a spiral line, as Mr. Newton seemed to suppose, but an excentrical elliptoid [*sic*], supposing no resistance in the medium: but, supposing a resistance, it would be an excentric ellipti-spiral, which, after many revolutions, would rest at last in the center; that the fall of the heavy body would not be directly east, as Mr. Newton supposed, but to the south-east, and more to the south than the east. It was desired that what was tryable in this experiment might be done with the first opportunity."

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[22] *Ibid.*, pp. 152-153.

[23] Birch, *op. cit.*, Vol. III, p. 516; Rouse Ball, *op. cit.*, p. 146; Rigaud, *op. cit.*, pp. 12-13. This extract has already been given, in rather different words of Waller's, in § VII of my article on Hooke (*Monist*, July, 1913).

Besides this, Rigaud[24] wrote:

"On the 11th of Dec., 1679,[25] he [Hooke] had explained that the body in its fall would be carried towards the south as well as towards the east, and this he described as having occurred in actual trial; but at the same time it is to be acknowledged that the coincidence must have been the consequence of accidental circumstances, since the fall, being only 27 feet, was too small to produce a deviation which he could have appreciated."

With regard to the further history of Newton's problem, we may remark, after Rigaud,[26] that it was treated by Thomas Simpson[27] and d'Alembert.[28] The experiment has also been tried with success by Gulielmini, Henzenberg, and Flaugergues, from stations of very great height; but still there are so many difficulties attending it that Delambre[29] considered it as by no means conclusive. It was found, indeed, by mathematicians that the deviation to the south is exceedingly small for any case in which it can be actually tried.

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[24] *Op. cit.,* p. 44.

[25] Birch, *op. cit.,* Vol. III, p. 11.

[26] *Op. cit.,* pp. 44-45.

[27] *Mathematical Dissertations,* 1743, p. 160.

[28] *Histoire de l'Académie de Paris,* 1771, p. 10.

[29] *Astronomie,* Vol. II, p. 192.

II

We now come to the question of the spiral.[1] If the earth were at rest, a heavy body would fall in a straight line towards its center, but if the motion communicated by the rotation acts as a projectile force, the combination of it with gravity would, Newton at first hastily stated, make the body descend in a spiral. Biot[2] said that doubtless Newton came to this conclusion because he supposed the fall to be in a resisting medium like the air, and this, at first sight, seems very probable, especially when we consider what is laid down in the fourth section of the second book of the *Principia*. But, said Rigaud, Newton said nothing to confirm this conjecture[3]: on the contrary, he rested his defence upon being engaged on other pursuits, so that "I thought no further of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time." Indeed the proposition was probably not communicated as the result of his own speculations, but as it casually occurred to his recollection in the act of writing. He had not then demonstrated the law of central forces, and the early impression of their effect seems very generally to have been that revolving bodies might ultimately fall down to the point to which they were attracted. This naturally led to the idea of a spiral path. Leonardo da Vinci had assigned it to them, and it had been maintained nearer to Newton's own time and under circumstances which were more likely to be present to his thoughts. There had been a controversy, in which the arguments derived by Riccioli from falling bodies against the earth's motion were answered by Stephanus de Angelis. An account of what passed between them was drawn up by James Gregory, and published in the second volume of the *Philosophical Transactions.* Mention was made, at the end, of the experiment tried by Descartes of discharging cannon balls perpendicularly in the air, and of their falling back to the surface more frequently to the west than to the east. This experiment, possibly too difficult of accurate execution to be at all relied upon, was brought forward in confirmation of the earth's motion, and it may have suggested that which Newton recommended for the same purpose. As De Angelis had argued against the false position of his antagonist, his views may have been received with the more favor. Now he assumed the spiral motion, although it is not noticed by Gregory, as we see by the preface to his tract *De infinitis spiralibus inversis.*[4]

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[1] Rigaud, *op. cit.*, pp. 45-47.

[2] In his biography of Newton in the *Biographie Universelle,* Vol. XXXI, p. 149; Eng. Trans., p. 14.

[3] Rigaud had not seen Newton's letter to Halley of May 27, 1686, mentioned in one of the preceding notes and confirming Biot's conjecture.

Among David Gregory's papers there is, said Rigaud,[5] a memorandum that "Mr. Newton pursued the motion of the planets first on supposition of an equal gravity, but afterwards wrote to Mr. Hooke, asking what he suspected the law of gravity to be." That Newton, before 1666, may at first have entertained such an idea of gravitation as is here described is very possible; but it is most evident, from Hooke's own statement, that his letter in 1679 originated with himself, and was no reply to any application which Newton had made to him.

III.

The line of argument which Newton adopted in this correspondence with Hooke supplies, said Rigaud,[1] a confirmation of his not having yet made use of Picard's measures for the correction of his first calculations. If he had himself "experimentally verified" his hypothesis, he would have hardly failed to mention it when he repeatedly pointed out Hooke's deficiency in this respect; and he distinctly said that, "before a certain demonstration" which he found in 1685, he had suspected that the law of the inverse squares "did not reach accurately enough down so low" as "the superficies of the earth," "and therefore in the doctrine of projectiles never used it nor considered the motion of the heavens." However this may be, he clearly followed his own precise rule of philosophizing: "In experimental philosophy we are to look upon propositions collected by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur by which they may either be made accurate or liable to exceptions."

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[4] Pat. 1667: "Jam anni quadrans evolvit, ex quo discursus quidam nostri Italice conscripti prodiere; in quibus rationes aliquot physico-mathematicas contra Coperniceum systema a viro eximio Ricciolio excogitatas, ad trutinam revocavimus. Affirmabamus tunc bina genera infinitarum spiralium a nobis fuisse contemplata; spatiaque ab ipsis clausa geometrice mensurata; quarum una illa foret, quae a gravi naturaliter cadente in plano aequatoris describeretur, is Tellus, ex falsa hypothesi, motu diurno duntaxat moveretur, et grave deorsum latum taliter suum concitaret motum, ut spatia, ab ipso peracta, forent ad invicem ut ipsa quadrata temporum." See also p. 17, and *Cartesii Epist.*, Part. prim., pp. 162, 173.

[5] *Op. cit.*, p. 47. [1] *Op. cit.*, pp. 48, viii.

Hooke's process, said Rigaud,[2] was much less strict; and, although it enabled him to take one great step towards the truth, it did not put him in possession of it either completely or with certainty. He saw the analogy between the moon's motion and that of the primary planets, and accordingly extended the law of the inverse squares to terrestrial gravitation, without knowing that this law of the effect ceased at the surface of the earth. He had left the verification of his theory to his readers in 1674, which made Newton very justly complain[3] that he should claim the honor of discovery for himself, and throw upon others the drudgery of ascertaining whether after all it was applicable to the facts. It is probable that he had only adopted the precise law of variation when he wrote to Newton in 1679. Hooke satisfied himself "that the proportion was duplicate *quam proxime* at great distances from the center, and only guessed it to be so accurately,"[4] but without Newton's demonstrations, to which he was a stranger, he had advanced no further than to an imperfect though beautiful conjecture. It may likewise be added that he had not that clear and steady view of his own first principles which, if he had attempted it, would have enabled him to arrive at more certain conclusions. In a lecture which he delivered as late as the 25th of May, 1687, he said: "I know that if the gravitating power in the sun and moon be exactly the same with that of the earth, the query I propounded can have no ground; but, though they may in most particulars be consonant, as I shall prove in my theory of gravity, yet there may be a cause (and there seems to be some assignable) why there may be something specific in each of them."

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[2] *Op. cit.*, pp. 49-50, viii.

[3] Letter to Halley of June 20, 1686; Rouse Ball, *op. cit.*, p. 159.

Arguing from the analogies which he had adopted, Hooke rejected Newton's suggestion of a spiral motion for the projectile, and said that its path would be an elliptical curve, but he did not venture the assertion of its being an ellipse: he only called it an eccentrical ellipsoid; and added that, supposing the motion to take place in a resisting medium, the path would become an "eccentric ellipti-spiral."

IV.

Hooke certainly saw that the earth must be oblate at the poles; mention was made of this theory in many parts of his writings, and particularly in his *"Discourse on Earthquakes,"* which Waller, the editor of his *Posthumous Works,* considered to have been written before 1667. He considered the solid part of the earth to be an exterior shell, and reasoned as to the figure which it assumed, from the different degrees of centrifugal force of the parts at different distances from the axis of rotation. Now this was a fact of which Newton did not need to be informed, for, in comparing the moon's orbit in 1666 with the effect of gravity, he calculated (as he said) "the force of ascent at the equator arising from the earth's motion"; and he resolved the problem more completely than it had ever been contemplated by Hooke, who assumed as the foundation of his argument that "the gravitating power of the earth be everywhere equal, as I know no reason to suppose the contrary"; and said that the waters of the sea may, at the poles, have a plane, if not a concave surface.[1]

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[4] *Ibid.,* p. 158.

V.

Newton replied to Hooke's letter of Dec. 9, but the reply is lost.[1] When Hooke said that the path was not a spiral but an "eccentrical elliptoid," or as Newton put it in a letter to Halley of 1686,[2] "Mr. Hooke replied it would not descend to the center but at a certain limit return upwards again. I then took the simplest case for computation, which was that of gravity uniform in a medium not resisting, imagining he had learned the limit from some computation, and for that end had considered the simplest case first. And in this case I granted what he contended for, and stated the limit as nearly as I could." In his reply of Jan. 6, 1680,[3] Hooke began:

"Your calculation of the curve described by a body attracted by an equal power at all distances from the center, such as that of a ball rolling in an upward concave cone, is right, and the two auges will not unite by about a third of a revolution; but my supposition is that the attraction always is in duplicate proportion to the distance from the center reciprocal, and consequently that the velocity will be in a subduplicate proportion to the attraction, and consequently, as Kepler supposes, reciprocal to the distance. and that with such an attraction the auges will unite in the same part of the circle, and that the nearest point of the access to the center will be opposite to the farthest distant, which I conceive doth very intelligibly and truly make out all the appearances of the heavens. And therefore (though in truth I agree with you that the explicating the curve in which a body descending to the center of the earth would circumgyrate were a speculation of no use, yet) the finding out the properties of a curve made by two such principles will be of great concern to mankind, because the invention of the longitude by the heavens is a necessary consequence of it, for the composition of two such motions I conceive will make out that of the moon. What I mentioned in my last concerning the descent within the body of the earth was but upon the supposal of such an attraction, not that I really believe there is such an attraction to the very center of the earth, but, on the contrary, I rather conceive that the more the body approaches the center the less will it be urged by the attraction, possibly somewhat like the gravitation on a pendulum or a body moved in a concave sphere where the power continually decreases the nearer the body inclines to a horizontal motion which it has when perpendicular under the point of suspension or in the lowest point, and there the auges are almost opposite, and the nearest approach to the center is at about a quarter of a revolution. But, in the celestial motions, the sun, earth, or central body is the cause of the attraction, and, though they cannot be supposed mathematical points, yet they may be conceived as physical, and the attraction at a considerable distance may be computed according to the former proportion as from the very center. This curve truly calculated will show the error of those many lame shifts made use of by astronomers to approach the true motions of the planets with their tables. But of this more hereafter."

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[1] Rigaud, *op. cit.*, p. 67.

[1] Rouse Ball, *op. cit.*, pp. 146-147.

[2] *Ibid.*, p. 155.

[3] *Ibid.*, p. 147.

Hooke then[4] gave an account of his trials of the "noble experiment" of the falling body, and added: "If it doth succeed there will follow several other consequences not less considerable—as, first, that all bodies will, in consequence, grow lighter the nearer they approach the equinoctial, the circular motion being the swifter, and for the same reason the further a body is from the center the less will be its gravitation, not only on account of the decrease of the attractive power, which I have a long time supposed, but upon the increase of the endeavor of recess. And this gives us another way to try whether the earth has a diurnal motion, though much short of what you proposed. But that I may tell you somewhat of observation, Mr. Halley, when he returned from St. Helena, told me that his pendulum at the top of the hill went slower than at the bottom, which he was much surprised at, and could not imagine a reason. But I presently told him that he had solved me a query I had long desired to be answered but wanted opportunity, and that was to know whether the gravity did actually decrease at a greater height from the center. To examine this decrease of attraction I have formerly made many experiments on Paul's steeple and Westminster Abbey, but none that were fully satisfactory. This will spoil the universal standard by the pendulum and the equality of pendulum clocks carried from one climate to another."

Newton said of this letter in 1686:[5]

"He replied that gravity was not uniform but increased in descent to the center in a reciprocal duplicate proportion of the distance from it, and thus the limit would be otherwise than I had stated it, namely, at the end of every entire revolution, and added that, according to this duplicate proportion, the motions of the planets might be explained and their orbs defined. This is the sum of what I remember."

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[4] *Ibid.*, p. 148; cf. pp. 148-149.

[5] *Ibid.*, p. 155.

Later he remembered something else:[6]

"He told me that my proposed experiment about the descent of falling bodies was not the only way to prove the motion of the earth; and so added the experiment of your pendulum clock at St. Helena as an argument of gravity's being lessened at the equator by the diurnal motion. The experiment was new to me, but not the notion; for, in that very paper which I told you was writ some time above fifteen years ago, I calculated the force of ascent at the equator, arising from the earth's diurnal motion, in order to know what would be the diminution of gravity thereby. But yet to do this business right is a thing of far greater difficulty than I was aware of."

Hooke again referred to the problems on orbits under attraction in a letter of Jan. 17, 1680,[7] where also the experiments were further referred to,[8] and then the correspondence on this subject closed.

With the controversy[9] which arose out of this correspondence, I shall not deal in any detail. The facts on which Hooke's claims were based are given above and in the following references.

With Hooke's *Attempt to prove the Annual Motion of the Earth* of 1674, which had been previously read to the Royal Society at Gresham College, I have already dealt in section V of "Robert Hooke as a Precursor of Newton."[10] His *De Cometa* of 1678 was dealt with in section VI, and his early paper of 1666 and a pendulum experiment in section III.

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[6] *Ibid.*, p. 165.

[7] *Ibid.*, p. 149.

[8] Cf. *ibid.*, p. 150.

[9] Cf. Hooke's notes (*ibid.*, pp. 150-153, 144-145); Newton's correspondence of 1686 with Halley (*ibid.*, pp. 154-155, 156-167); *ibid.*, pp. 68-73; and Rigaud, *op. cit.*, pp. 37-71.

[10] *Monist*, July, 1913..

Hooke wrote, in the letter[11] of Sept. 15, 1689, which was to pass for Aubrey's to Antony Wood, that he wrote to Newton "to make a demonstration of *this theory* [in Hooke's *Attempt*] *not* telling him *at first* the proportion of the gravity to the distance, [and] *nor what was* the curved line that was thereby made.

"Mr. Newton, in his answer to the letter, *did express* that he had not thought of it; and in his first attempt about it, he calculated the curve by supposing the attraction to be the same at all distances."

Thus Newton was led to consider what would be the form of the curve which a body projected from a point and acted upon by a central attraction varying inversely as the square of the distance would describe. Then too he established the theorem relating to the equal description of areas, and discovered the method of determining the law of central force in order that any given orbit might be described under its action. He applied this to the ellipse and proved that this curve would be described under the action of a force in the focus varying inversely as the square of the distance.[12]

VI.

According to J. W. L. Glaisher, in his address delivered on the bicentenary of the publication of the *Principia,* Newton's thoughts were, in 1666, carried from the earth to the moon. Kepler's laws had reference to the sun and planets, so we may safely assume that Newton's mind had grasped the idea of a single principle of gravitation governing the motions of bodies on earth and in the heavens.

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[11] Rigaud, *op. cit.,* App. No. XIII, p. 52. Hooke's interlineations are printed in italics.

[12] This is from Glaisher's account referred to below, which is partly based on the manuscript note of Newton's reproduced above from Rouse Ball's book, pp. 6-7. Cf. Rouse Ball, *op. cit.,* pp. 20-21.

This appears to be similar to the opinion of Adams referred to above. In this view there are three distinct steps in Newton's great discovery; the first is physical and the other two are mathematical. The first is the conception, that was present to Newton's mind in 1666, that all the particles of matter in the earth and the moon attract each other according to the law of the inverse square; the second is the mathematical investigations of 1679; and the third is the theorem of 1685 on the attraction of a sphere.

By this determination of the attraction of a sphere on an external particle,[1] and by the unexpected—as Newton himself said—finding that this attraction is directed to the center of this sphere, Newton saw, as Glaisher[2] has put it, all the mechanism of the universe at once spread out before him. "When he discovered the theorems that form the first three sections of Book I, when he gave them in his lectures of 1684, he was unaware that the sun and earth exerted their attractions as if they were but points. How different must these propositions have seemed to Newton's eyes when he realized that these results, which he had believed to be only approximately true when applied to the solar system, were really exact! Hitherto they had been true only in so far as he could regard the sun as a point compared to the distance of the planets or the earth as a point compared to the distance of the moon—a distance amounting to only about sixty times the earth's radius—but now they were mathematically true, excepting only for the slight deviation from a perfectly spherical form of the sun, earth, and planets. We can imagine the effect of this sudden transition from approximation to exactitude in stimulating Newton's mind to still greater efforts. It was now in his power to apply mathematical analysis with absolute precision to the actual problems of astronomy."

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[1] Rouse Ball, *op. cit.*, pp. 11, 35, 61, 157.

[2] *Ibid.*, p. 61.

The discovery of the theorem by which the attraction of a spherical mass is determined was thus rightly emphasized by Glaisher—as it was by Edleston[3] — as an immensely important—perhaps in a sense the most important —of Newton's researches on gravitation. But when Newton said, as he did in the letter of 1686 already quoted,[4] that he never "considered the motions of the heavens" before he discovered this theorem in 1685, his statement is so plainly wrong that an "it in"—where the "it" refers to the law of attraction below the surface of the attracting body—must have been omitted by mistake between the "considered" and the "the."

Hooke, in his paper read to the Royal Society on March 21, 1666,[5] said:[6] "If all the parts of the terrestrial globe be magnetical, then a body, at a considerable depth below the surface of the earth, should lose somewhat of its gravitation or endeavor downwards by the attraction of the earth placed above it. This opinion some experiments made by some worthy persons of this honorable society seem to countenance"; but he pointed out the difficulty of conducting them in a conclusive manner and referred to the trial by clocks at certain depths and heights which, as Whewell[7] remarked, had been previously suggested by Bacon.

When Newton's temper was roused, he did not delay to imagine that Hooke might be a plagiarist;[8] but he was easily pacified.[9] Still, even then, he did not allow generosity to occupy too large a place in his just acknowledgments.[10] Newton's suspicions seem to have been very easily roused with regard to both enemies and friends. Worse than this is the way in which Newton, doubtless in one of his irritable fits, laid implicit or explicit claim to merits which he did not possess at the time under discussion. In a more generous moment he admitted[11] that Hooke was correct about the spiral; but, in the angry postscript, he said:[12]

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[3] *Op. cit.*, p. xxix. In 1685, said Edleston, Newton "determines the attractions of masses and thus completes the demonstration of the law of universal gravitation."

[4] Rouse Ball, *op. cit.*, p. 157.

[5] See Section II of "Robert Hooke as a Precursor of Newton," *Monist*, July, 1913. Cf. Rigaud, *op. cit.*, pp. 56-58.

[6] Birch, *op. cit.*, Vol. II, p. 70.

[7] *History of the Inductive Sciences*, Vol. II, p. 173.

[8] Rouse Ball, *op. cit.*, p. 160.

[9] *Ibid.*, p. 165.

"For his extending the duplicate proportion down to the center (which I do not) made him correct me, and tell me the rest of his theory as a new thing to me, and now stand upon it that I had all that from his letter, notwithstanding that he had told it to all the world before, and I had seen it in his printed books, all but the proportion."

Does not this imply that Newton was too cautious to consider the path below the surface of the earth? We know, from the letter he wrote to Hooke in 1679 and which is reproduced above, that he was not so cautious then. He had no inkling then of what he knew in 1686.

We must remember that this is not an isolated case. If it were, we would not suspect Newton of a slight meanness on no better evidence. But to throw discredit on Leibniz's infinitesimal calculus, he denied that he had ever used infinitesimal methods. On this De Morgan[13] remarked:

"We are of opinion that the *moral intellect* of Newton—not his moral *intention*, but his power of judging—underwent a gradual deterioration from the time when he settled in London. We see the faint traces of it in his manner of repudiation of the *infinitesimal* view of fluxions, in 1704. A man of sound judgment as to what is right does not abandon a view which he has held in common with a great rival, and this just at a time when the world is beginning to ask which came first in their common discovery, without a clear admission of the abandonment: he does not imply that *some* have held that view, and declare against the opinion of those *some,* without a distinct statement that he himself had been one of them: still less does he quietly and secretly alter what he had previously published, or allowed to be published, so as to turn the old view into the new one, and to leave the reader to understand that he had never changed his opinion. The Newton of the mythologists would have felt to his fingers' ends that such a proceeding had a tendency to give false impressions as to the case, and to throw suspicion on his own motives. This is a small matter, but it is a commencement of worse. We come to the *Commercium epistolicum,* the name given to the collection of letters accompanied by notes and a decision of the question, on the part of a Committee of the Royal Society. To this well-known part of the history Sir D. Brewster has a very important addition to make; and he makes it fairly, though we confess we wish he had given us what they call chapter and verse. 'It is due to historical truth to state that Newton supplied all the materials for the *Commercium epistolicum,* and that, though Keill was its editor, and the Committee of the Royal Society the authors of the Report, Newton was virtually responsible for its contents.' "

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[10] *Ibid.*, pp. 165-167.

[11] *Ibid.*, pp. 165, 167. See above, §§ I-III, V.

[12] *Ibid.*, p. 160.

[13] Review of Brewster in *North British Review*, Vol. XXIII, 1855, pp. 324-325. This review is included in De Morgan's *Essays on the Life and Work of Newton*, just published by the Open Court Publishing Company.

VII.

In the angry postscript to his letter written to Halley on June 20, 1686,[1] Newton seems to have laid his finger on the exact spot of Hooke's failure and his success:

"For it is plain, by his [Hooke's] words, that he knew now how to go about it. Now is not this very fine? Mathematicians that find out, settle, and do all the business, must content themselves with being nothing but dry calculators and drudges; and another that does nothing but pretend and grasp at all things must carry away all the invention, as well of those that were to follow him as of those that went before. Much after the same manner were his letters writ to me, telling me that gravity, in descent from hence to the center of the earth, was reciprocally in a duplicate ratio of the altitude, that the figure described by projectiles in this region would be an ellipsis, and that all the motions of the heavens were thus to be accounted for; and this he did in such a way as if he had found out all, and knew it most certainly. And upon this information I must now acknowledge in print that I had all from him, and so did nothing myself but drudge in calculating, demonstrating, and writing, upon the inventions of this great man."

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[1] Rouse Ball, *op. cit.,* pp. 159-160.

VIII.

We now leave Hooke and come to the incidents of 1684.

As Newton observed in the scholium to the fourth proposition of the first book of the *Principia,* Sir Christopher Wren, Hooke, and Halley had each observed that, in the case of the celestial bodies, "if the periodic times are in the sesquiplicate ratio of the radii, and therefore the velocities reciprocally in the subduplicate ratio of the radii, the centripetal forces will be in the duplicate ratio of the radii inversely: and the contrary." The facts were given in a letter from Halley to Newton dated June 29, 1686:[1]

"According to your desire in your former, I waited upon Sir Christopher Wren to inquire of him if he had the first notion of the reciprocal duplicate proportion from Mr. Hooke. His answer was that he himself very many years since had had his thoughts upon the making out of the planets' motions by a composition of a descent towards the sun and an impressed motion; but that at length he gave over, not finding the means of doing it. Since which time Mr. Hooke had frequently told him that he had done it, and attempted to make it out to him, but that he never satisfied him that his demonstrations were cogent. And this I know to be true, that in January, 1683/4 I, having from the consideration of the sesquialter proportion of Kepler, concluded that the centripetal force decreased in the proportion of the squares of the distances reciprocally, came one Wednesday to town, where I met with Sir Christopher Wren and Mr. Hooke, and, falling into discourse about it, Mr. Hooke affirmed that upon that principle all the laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill success of my attempts; and Sir Christopher, to encourage the inquiry, said that he would give Mr. Hooke or me two months' time to bring him a convincing demonstration thereof, and, besides the honor, he of us that did it should have from him a present of a book of forty shillings. Mr. Hooke then said that he had it, but he would conceal it for some time, that others, trying and failing, might know how to value it when he should make it public. However, I remember Sir Christopher was little satisfied that he could do it, and though Mr. Hooke then promised to show it him, I do not yet find that in that particular he has been as good as his word. The August following, when I did myself the honor to visit you, I learnt the good news that you had brought this demonstration to perfection, and you were pleased to promise me a copy thereof, which, the November following, I received with a great deal of satisfaction from Mr. Paget; and thereupon took another journey down to Cambridge on purpose to confer with you about it, since which time it has been entered upon the register books of the Society. As all this time past Mr. Hooke was acquainted with it, and, according to the philosophically ambitious temper he is of, he would, had he been master of a like demonstration, no longer have concealed it, the reason he told Sir Christopher and me now ceasing. But now he says that this is but one small part of an excellent system of nature which he has conceived, but has not yet completely made out, so that he thinks not fit to publish one part without the other. But I have plainly told him that unless he produce another differing demonstration and let the world judge of it, neither I nor any one else can believe it."

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[1] Brewster, *op. cit.,* Vol. I, pp. 293, 296-298; Rouse Ball, *op. cit.,* pp. 25-36, 162-163.

When in the August of 1684[2] Halley[3] visited Newton, he, without mentioning any of these speculations, "at once indicated the object of his visit by asking Newton what would be the curve described by the planets on the supposition that gravity diminished as the square of the distance. Newton immediately answered, 'an ellipse.' Struck with joy and amazement Halley asked him how he knew it. 'Why,' replied he, 'I have calculated it'; and, being asked for the calculation, he could not find it, but promised to send it to him. After Halley left Cambridge, Newton endeavored to reproduce the calculation, but did not succeed in obtaining the same result. Upon examining carefully his diagram and calculation, he found that in describing an ellipse coarsely with his own hand he had drawn the two axes of the curve instead of two conjugate diameters somewhat inclined to one another. When this mistake was corrected he obtained the result which he had announced to Halley."[4]

Newton sent the propositions, which were presumably on the conservation of areas under a central force, the law of force in a focus under which an ellipse can be described, and the converse on the orbit described by a particle under a central force which varies inversely as the square of the distance,[5] to Halley in November, 1684; and Halley, on his return from his second visit to Newton, gave at the meeting of the Royal Society on Dec. 10, "an account that he had lately seen Mr. Newton at Cambridge, who had showed him a curious treatise, *De motu*; which, upon Mr. Halley's desire, was, he said, promised to be sent to the Society to be entered upon their register. Mr. Halley was desired to put Mr. Newton in mind of his promise for the securing his invention to himself till such time as he could be at leisure to publish it."[6]

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[2] On the origin of the mistake sometimes made of taking 1683 for 1684, see Brewster, *op. cit.*, Vol. I, note on pp. 296-297; Rouse Ball, *op. cit.*, pp. 32-33.

[3] It must be remembered that an immense debt is owing to Edmund Halley both for the unwearied tact which he showed in persuading Newton to overcome his reluctance to publish, and for his generosity in paying for the publication when he could ill afford it (*ibid.*, pp. 67-68).

[4] Brewster, *op. cit.*, p. 297; Rouse Ball, *op. cit.*, p. 26.

Newton seems to have been stimulated by Halley's visit in August to continue his investigations. In the Michaelmas term of 1684 he read, as his professorial lectures, from a manuscript which is still extant and which bears the title *De motu corporum.* Whether this was the "curious tract" seen by Halley, or whether Halley saw the *Propositiones de motu,*[7] which was afterwards registered on the books of the Royal Society, is a question which has been much discussed,[8] but which is unimportant from the point of view of the development of the principles of mechanics. For the fact is that, in the first three lectures, the definitions and laws of motion are given exactly in the form in which they were stated in the *Principia.* Of these lectures it might be said with far more justification than has been said by Rouse Ball[9] of the tract *Propositiones de motu,* that they are "memorable as marking the point at which Newton had arrived about the end of 1684." I will discuss

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[5] Rouse Ball, *op. cit.,* pp. 26-27. The propositions are I, XI, and XIII (Cor. I) of the first Book of the *Principia.*

[6] Rouse Ball, *op. cit.,* p. 30.

[7] See Rouse Ball, *op. cit.,* pp. 31, 33-51 (where it is printed).

[8] See *ibid.,* pp. 30-33; Brewster, *op. cit.,* pp. 299-300.

[9] *Op. cit.,* p. 33.

Newton's lectures in the next section; in the following section, the parts of the above tract which relate to the principle and the manuscript drafts of it in the Cambridge University Library will be described.

Here may be put in a small bit of evidence that Newton did not send the tract to Halley in November, but only an informal communication. Newton wrote to Halley on May 27, 1686: "In the papers in your hands there is not one proposition to which he [Hooke] can pretend."[10]

IX.

Newton had been appointed Lucasian Professor in the University of Cambridge in 1669, and first lectured on mechanics in the Michaelmas term of 1684.[1] The manuscript of these lectures is preserved at the University Library in Cambridge[2] in a volume containing the book-plate of King George the First, probably inserted by mistake, as the volume could not have been part of Bishop Moore's library.[3]

The reason of the appearance of King George the First's book-plate in the books from Bishop Moore's library in the University Library of Cambridge is as follows:[4] In 1715 the university sent up a loyal address to King George I on his accession, and the king, as a reward, bought the library of Dr. John Moore (1646-1714, Bishop of Norwich and afterwards of Ely), and presented it to the university. About the same time a troop of horse was quartered at Oxford in consequence of a Jacobite disturbance on the anniversary of the Pretender's birth. Hence the following epigram by an Oxford wit, probably Dr. Trapp or Thomas Warton:

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[10] *Ibid.*, p .155.

[1] On these and Netwon's other lectures, see Rouse Ball, *op. cit.*, pp. 27-30.

[2] Class-mark: "Dd. 9. 46." The Contents are described by Edleston (*op. cit.*, pp. xcv-xcvii) and Rouse Ball.

[3] Henry Bradshaw (*Collected Papers of Henry Bradshaw, late Universty Librarian,* Cambridge, 1889, p. 201) wrote that there are no inventories of Bishop Moore's library preserved, not even of the manuscripts.

[4] *In Praise of Cambridge*: *an Anthology in Prose and Verse,* selected and edited by Sydney Waterlow, London, 1912, pp. 14, 210.

"The king observing with judicious eyes,
The state of both his universities,
To one he sends a regiment; for why?
That learned body wanted loyalty.
To th'other books he gave, as well discerning
How much that loyal body wanted learning."

Dr. William Browne (1692-1774), according to Mrs. Piozzi, heard Dr. Samuel Johnson quote this, and thereupon composed impromptu:

"The king to Oxford sent his troop of horse,
For Tories own no argument but force.
With equal care, to Cambridge books he sent:
For Whigs allow no force but argument."

"Mr. Johnson," says Mrs. Piozzi, "did Browne the justice to say it was one of the happiest extemporaneous productions he ever met with; though he once comically confessed that he hated to repeat the wit of a Whig in support of Whiggism."

X.

These lectures contain the definitions, laws of motion, the first section, and the first 35 propositions of the *Principia*. It must be remembered that the third law of motion of the *Principia* is not used until the eleventh section of that work, which deals with the motion of two or more bodies under their mutual attractions, and begins with Proposition LVII. In the tract sent to the Royal Society, there is, as we shall see in the next section, no mention at all of the third law; but there is in one of the unpublished drafts of this manuscript. From another manuscript in the Portsmouth Collection, which has not, I think, been particularly noticed before, there is a statement of the third law which seems to carry its recognition back to an earlier period of Newton's life.

Brewster[1] merely said of the Royal Society's tract that "it occupies twenty-four octavo pages, and consists of four theorems and seven problems, four of the theorems and four of the problems containing the more important truths which are demonstrated in the second and third sections of the first book of the *Principia*"; and neither by Brewster, Rosenberger,[2] nor by any historian of mechanics or mathematics, so far as I know, is pointed out the fact that the third law of motion appears not at all in the Royal Society's manuscript; or that it appears in a different form to that of the *Principia*, in a manuscript in the Portsmouth Collection written shortly after this.

XI.

In the Royal Society's tract[1] there are three definitions, (centripetal force, force of a body [of inertia], and resistance) and[2] four "Hypotheses":

"1. In the next nine propositions the resistance is to be zero, in the following ones it is to be conjointly as the density of the medium and the velocity;

"2. Every body, by the force implanted in it, moves to infinity uniformly in a straight line unless something outside it prevents it;

"3. A body in a given time, under the joint action of forces, is carried to the same place as by separate forces in successive equal times;

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[1] *Op. cit.*, Vol. I, p. 299. Cf. Rosenberger, *op. cit.*, p. 159.

[2] When speaking of the third law of motion on pp 176-177 (*op cit.*).

[1] Reproduced on pp. 35-51 of Rouse Ball's book.

[2] "1. Resistentiam in proximis novem propositionibus nullam esse, in sequentibus esse ut medii densitas et celeritas conjunctim.

"2. Corpus omne sola vi insita uniformiter secundum lineam rectam in infinitum progredi, nisi aliquid extrinsecus impediat.

"3. Corpus in dato tempore, viribus conjunctis, eo ferri quo viribus divisis in temporibus aequalibus successive.

"4. Spatium quod corpus, urgente quacunque vi centripeta, ipso motus initio describit, esse in duplicata ratione temporis" (*ibid.*, p. 36).

"4. The space which a body, being urged by some centripetal force, describes from the same beginning of motion, is in the duplicate ratio of the time."

The propositions correspond[3] to those in the *Principia,* Book I, Propositions 1, 4, 6, 7, 10, 11, 15, 17, 32, and Book II, Propositions 2, 3, and 4, and deal with the motion of particles about fixed centers of force in non-resisting and resisting media. There is nothing corresponding to the third law of motion here.

But there is in the Portsmouth Collection[4] a manuscript draft of the tract, which appears to me to be of later date than the other two. It is somewhat fuller and bears the heading, which perhaps is of a later date than the manuscript and possibly not in Newton's handwriting: *De motu sphaericorum corporum in fluidibus,*[5] and contains a different form of the third law of motion. In this manuscript there are five laws,[6] the term "hypothesis" here and in the subsequent references in the manuscript being crossed out and *lex* written. These are as follows:[7]

"Law 1. A body always proceeds, by the force implanted in it alone, uniformly in a straight line if nothing prevents it;

"Law 2. Change of state of motion or rest is proportional to the impressed force and takes place in the straight line in which that force is impressed;

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[3] Cf. *ibid.,* pp. 33-34.

[4] On two other drafts of the tract, see *ibid.,* p. 51.

[5] Reproduced on pp. 51-56 (*ibid.*).

[6] *Ibid.,* p. 52. Cf. the application of the third and fourth laws translated below (from pp. 54-55).

[7] "Lex 1. Sola vi insita corpus uniformiter in linea recta semper pergere si nil impediat.

"Lex 2. Mutationem status movendi vel quiescendi proportionalem esse vi impressae et fieri secundum lineam rectam qua vis illa imprimitur.

"Lex 3. Corporum dato spatio inclusorum eosdem esse motus inter se sive spatium illud quiescat sive moveat id perpetuo et uniformiter in directum absque motu circulari.

"Lex 4. Mutuis corporum actionibus commune centrum gravitatis non mutare statum suum motus vel quietis. Constat ex Lege 3.

"Lex 5. Resistentiam medii esse ut medii illius densitas, et corporis moti sphaerica superficies et velocitas conjunctim" (*ibid.* p. 52).

"Law 3. The motions with respect to one another of bodies included in a given space are the same whether the space is at rest or moves perpetually and uniformly in a straight line without a circular motion;

"Law 4. By the mutual action of bodies the common center of gravity does not change its state of motion or rest. Proved from Law 3.

"Law 5. The resistance of a medium is proportional conjointly to the density of the medium, the spherical surface of the body moved, and the velocity."

At the end of Law 4, after "Constat ex," apparently "ex lege 3" was written, crossed out, and then rewritten. Law 3 is the same as Cor. V and Law 4 as Cor. IV to the Laws of Motion of the *Principia.*

In this manuscript there is an addition[8] of which this is a translation:

"The whole remaining space of the planetary heavens either is at rest (as is commonly believed) or moves uniformly in a straight line, and therefore the common center of gravity of the planets (by Law 4) either is at rest or moves with it. In both cases the motions of the planets relative to one another take place (by Law 3) in the same manner, and their common center of gravity is at rest with respect to the whole of space and ought therefore to be considered as the motionless center of the whole planetary system. Hence, in truth, the Copernican system is proved *a priori.* For if the common center of gravity be computed for any position of the planets, it will either fall in the body of the sun or will always be near it. Owing to this deviation of the sun from the center of gravity, it comes about that the centripetal force does not always tend towards the motionless center, and consequently that the planets neither move exactly in ellipses nor revolve twice in the same orbit. There are as many orbits of each planet as there are revolutions, as is the case with the moon's motion, and each several orbit depends on the conjoint motions of all the planets, to say nothing of the mutual actions which they all exert on one another. But to consider at once so many causes of motions and to define the motions themselves by exact laws admitting of convenient calculation surpasses, if I mistake not, the power of any human ingenuity. Neglecting these details, the orbit will be simple, and, as the mean of all the errors, will be the ellipse of which I have treated already. If anyone should try to determine this ellipse from three observations by a trigonometrical computation (as is usual), he will have approached the matter without sufficient caution. The observations will participate in the minute irregularities of the motions which are here neglected, and will therefore cause the ellipse to deviate a little from its just magnitude and position (which ought to be the mean of all the errors), and will give as many ellipses discrepant with one another as sets of three observations are admitted. Therefore as many observations as possible ought to be conjoined and compared by one operation, that these operations may mutually correct one another and exhibit the ellipse as a mean in position and magnitude."

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[8] *Ibid.*, pp. 54-55.

XII.

We will now turn our attention to the other manuscripts of Newton's preserved in the University Library of Cambridge.

Among the early papers by Newton (holograph) in Section I, Division I, of the Portsmouth Collection[1] are:

3. A tract in English written in 1666 and entitled: *To Resolve Problems by Motion.* Also short tracts entitled:

De solutione problematum per motum;

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[1] *Portsmouth Catalogue,* p. 1. The numbers 3-5 refer to items in this *Catalogue.*

De gravitate conicarum;
Problems of Curves.

4. *Calculation of the Area of the Hyperbola.*[2]
5. *On the Laws of Motion;*
On the Laws of Reflection;
On Motion in a Cycloid.

The tract *To Resolve Problems by Motion* is dated—apparently by Newton at a later date—October, 1666, and is concerned, not with mechanics, but with the use of the conception of motion in geometry.

In the margins of the tract *On the Laws of Motion* in No. 5 are:

1. Of place, motion, velocity and force;
2. With what velocity a body moves several ways at once;
3. How two progressive motions are joined into one [Parallelogram of motions];
4. Of centers and axes of motion and the motion of those centers.
5. Of circular motion and velocity about those axes;
6. With what velocity a body circulates about several axes at once;
7. How two circular motions are joined into one;
8. In what cases a circulating body perseveres in the same state and in what it doth not.

How bodies are reflected.

9. Some names and letters defined;
10. The Rule for Reflection;
11. The Conclusion;
12. In what method the precedent rules must be used.

The tract concludes with "Some observations about motion." In the first section occur the words:

"Which motion is done with more or less velocity accordingly as it is done through more or less space in equal times or through equal spaces in more or less time. But the motion itself and the force to persevere to the motion is more or less accordingly as the factors of the body's bulk into its velocity is more or less. And that force is equivalent to that motion it is able to beget or destroy."

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[2] This was an early calculation of Newton's performed at Boothby in Lincolnshire.

Again, the fourth section is:

"In every body there is a certain point, called its center of motion, about which if the body be any way circulated, the endeavors of its points every way from the center are exactly counterpoised by opposite endeavors. And the progressive motion of the body is the same with the motion of this its center which always moves in a straight line and uniformly with other bodies. And so doth the common center of two bodies, which is found by dividing the distance between their proper centers in reciprocal proportion to their bulk. And so the common center of three or more bodies, etc. And all the lines passing through these centers of motion are axes of motion."

The tract *On the Laws of Reflection* begins in the same way as above.

In Section VIII, No. 5,[3] there is the beginning of "The Elements of Mechanicks." This tract begins as follows:

"1. All bodies continue perpetually in their state of resting or moving in right lines unless disturbed by any force, and, when disturbed, the changes of these states are proportional to the force disturbing them. And if this change and the antecedent motion of the body be represented in situation and quantity by the sides of a parallelogram, the motion arising from them both shall be represented by the diagonal of that parallelogram.

"2. The actions of two bodies upon one another are always equal and directly contrary, and the resiliation of clashing bodies is as their springiness, and the force of a globular body upon a resting plane is as the part of its velocity which is perpendicular to the plane.

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[3] *Portsmouth Catalogue,* p. 49.

"3. The forces with which the parts of one and the same engine move at one and the same time are reciprocally proportional to their velocities.

"4. In a system of bodies moving variously amongst one another and acting upon one another by any other bodies, the common center of gravity of them all either rests perpetually or moves uniformly in a right line, and, whether it rests or moves, the motions of the bodies amongst one another are the same.

"5. Falling bodies describe spaces proportional to the squares of the times, and so do projectiles by their motions of ascent and descent, but their horizontal motions are uniform and with both motions together they describe parabolas, and water spouts up in a parabolic figure after the manner of projectiles."

These extracts show that Newton's conception of mass as quantity of matter dated from his early days. Indeed, it is quite likely that this conception was possessed by others[4] than Newton before Newton's time, but in an unformulated fashion. If we examine the basis of this conception, we see that the conception rests upon the implicit postulate of the sameness, in essentials, of all matter.

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[4] I am thinking more particularly of Wallis. Besides the reference to him in "Robert Hooke as a Precursor of Newton" (*Monist,* July, 1913), cf. the following passage from Edleston, *op. cit.,* pp. 300-301, from a letter which Wallis wrote to Newton on April 10, 1695: "I understood you are now about adjusting the moon's motions; and, amongst the rest, take notice of that of the *common center of gravity* of the earth and moon as a conjunct body (a notion which, I think, was first started by me, in my Discourse of the Flux and Reflux of the Sea). And it must needs be of a like consideration in that of Jupiter with his Satellites, and of Saturn with his. (And I wonder we have not yet heard of any about the moon.) But, Saturn and Jupiter being so far off, the effects thereof are less observable by us than that of the moon. My advice upon the whole, is, that you would not be too slow in publishing what you do." In Siegmund Günther's article "Histoire des origines de la loi Newtonienne de la gravitation" (Bibliothèque du Congrès internationale de Philosophie, III: *Logique et histoire des sciences,* Paris, 1901, pp. 49-75), which is very rich in references, there is on p. 63 a statement that Fermat was acquainted with the principle of the equivalence of action and reaction, and also with the fact that a material point is more feebly attracted inside a sphere than outside.

There seems to be a natural bent of the human mind to start from, or arrive at, this postulate. Descartes, who had the philosopher's quality of being conscious of some at least of the postulates on which his system was founded, started from the hypothesis of a homogeneous *plenum,* and his example is being constantly followed by those physicists of even our own day who are unacquainted with the history of thought. Hooke, as we have seen, kept the *plenum* but believed in qualitative differences in matter. Newton seems to have believed in the ultimate sameness of constitution of matter, and hence must have early freed himself from Cartesian opinions, if he ever held them. He seems to have been a follower of Robert Boyle's "corpuscular philosophy."[5]

XIII.

In order to compare these earlier statements of Newton's fundamental principles of dynamics with those in his *Principia* of 1687, it will be as well to consult the translation by Andrew Motte[1] of Newton's final statement of the "Axioms or Laws of Motion."

I will only make two observations on this later form of Newton's Laws of Motion.

In that manuscript of Newton's which is the second dealt with in §XI above, what is there called "Law 4" is stated to be proved by "Law 3." Now, "Law 3" is the same as the fifth corollary to the Laws of Motion of the

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[5] On Descartes's physical theory and for a criticism of it, see Kurd Lasswitz, *Geschichte der Atomistik vom Mittelalter bis Newton,* Vol. II, Hamburg and Leipsic, 1890, pp. 55-126. By "density" (*soliditas*) Descartes (*Principia,* 1644, III, 121) understood the quantity (*quantitas*) of his "third element"—which forms the enveloping crust of the planets—as compared with its total magnitude (*moles*) and surface (see Lasswitz, *op. cit.,* pp. 70-72). On Boyle's corpuscular theory, see *ibid.,* pp. 261-293. On Huygens's work, see *ibid.,* pp. 341-397. Finally, on Newton's physics, see *ibid.,* pp. 555-580.

[1] *The Mathematical Principles of Natural Philosophy*; 2 vols. London, 1729. The laws of motion are on pp. 19-40 of the first volume. American editions, New York, 1848, 1850.

Principia: "The motions of bodies included in a given space are the same among themselves whether that space is at rest or moves uniformly forwards in a right line without any circular motion." Newton deduced this corollary from the second law of the *Principia*. "Law 4" of the manuscript is the same as the fourth corollary to the Laws of Motion of the *Principia*: "The common center of gravity of two or more bodies does not alter its state of motion or rest by the actions of the bodies among themselves; and therefore the common center of gravity of all bodies acting upon each other (excluding outward actions and impediments) is either at rest or moves uniformly in a right line." This corollary is a simple deduction from the third Law of the *Principia*, and cannot, as Newton appeared to think in the manuscript referred to, be deduced from the "Law 3" of that manuscript.

XIV.

In the second place, Newton had a most interesting passage in the scholium to the Laws of Motion. After showing that "the third law, so far as it regards percussions and reflexions, is proved by a theory exactly agreeing with experience," he proceeded as follows:

"In attractions I briefly demonstrate the thing after this manner. Suppose an obstacle is interposed to hinder the congress of any two bodies A, B, mutually attracting each other, then if either body, as A, is more attracted towards the other body B, than that other body B is towards the first body A, the obstacle will be more strongly urged by the pressure of the body A than by the pressure of the body B, and therefore will not remain in equilibrium: but the stronger pressure will prevail, and will make the system of the two bodies, together with the obstacle, move directly towards the side on which B lies; and in free space to go forward to infinity with a motion perpetually accelerated—which is absurd, and contrary to the first law. For, by the first law, the system ought to persevere in its state of rest or of uniform motion forwards in a right line; and therefore the bodies must equally press the obstacle, and be equally attracted one by the other. I made the experiment on the loadstone and iron. If these are placed apart in proper vessels and made to float by one another in standing water, neither of them will propel the other, but by being equally attracted they will sustain each other's pressure and rest at last in equilibrium.

"So[1] the gravitation between the earth and its parts, is mutual. Let the Earth FI be cut by any plane EG into two parts EGF and EGI: their weights one towards the other will be mutually equal. For if, by another plane HK, parallel to the former EG, the greater part EGI is cut into two parts EGKH and HKI, whereof HKI is equal to the part EFG first cut off: it is evident that the middle part EGKH will have no propensity by its proper weight towards either side, but will hang as it were at rest and in equilibrium between both. But the one extreme part HKI will with its whole weight bear upon and press the middle part towards the other extreme part EGF; and therefore the force with which EGI, the sum of the parts HKI and EGKH, tends towards the third part EGF, is equal to the weight of the part HKI, that is, to the weight of the third part EGF. And therefore the weights of the two parts EGI and EGF, one towards the other, are equal, as I was to prove. And indeed if those weights were not equal, since the whole earth floats in the non-resisting ether, it would give way to the greater weight, and, retiring from it, would be carried off to infinity."

This passage is particularly interesting because it contains the same form of argument—the excluded *perpetuum*

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[1] This paragraph first appeared in the second edition (1713) of the *Principia.*

mobile—that, as Mach[2] has shown, Stevinus, Galileo, Huygens, Torricelli, John Bernoulli, Lagrange, Sadi Carnot, Julius Robert Mayer, Franz Neumann, and Helmholtz used as the foundation both of mechanics and of other parts of physics. That this remark of Newton's was not referred to by Mach makes it all the more necessary to refer to it here.

Further to illustrate this point, we will consider a part of the correspondence of Cotes and Newton, when Cotes was preparing the second edition of the *Principia*.[3]

Cotes, when giving Newton, in a letter of March 18, 1713, an account of the Preface he meant to write, commented as follows on Prop. V of Book III:[4]

"But, in the first corollary of this fifth proposition, I meet with a difficulty; it lies in these words: *Et cum attractio omnis mutua sit.* I am persuaded that they are then true when the attraction may properly be so called, otherwise they may be false. You will understand my meaning by an example. Suppose two globes A and B placed at a distance from each other upon a table, and that, whilst the globe A remains at rest, the globe B is moved towards it by an invisible hand; a bystander, who observes this motion but not the cause of it, will say that the globe B does certainly tend to the center of globe A, and thereupon he may call the force of the invisible hand the centripetal force of B and the attraction of A, since the effect appears the same as if it did truly proceed from a proper and real attraction of A. But then I think he cannot, by virtue of the axiom *Attractio omnis mutua est*, conclude, contrary to his sense and observation, that the globe A does also move towards the globe B and will meet it at the common center of gravity of both bodies. This is what stops me in the train of reasoning by which I would make out, as I said, in a popular way, your seventh proposition of the third book. I shall be glad to have your resolution of the difficulty, for such I take it to be. If it appears so to you also, I think it should be obviated in the last sheet of your book, which is not yet printed off, or by an Addendum to be printed with the Errata table. For, until this objection be cleared, I would not undertake to answer any one who should assert that you do *hypothesim fingere*. I think you seem tacitly to make this supposition that the attractive force resides in the central body."

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[2] See the English translation of his publication of 1872. This translation is entitled: *History and Root of the Principle of the Conservation of Energy* (Chicago: Open Court Publishing Co., 1911).

[3] In all of these letters, since the interest is scientific rather than literary, I have modernized dates and spelling, and translated Latin sentences, for the convenience in reading and printing.

[4] Edleston, *op. cit.*, pp. 152-153, The proposition referred to is: "That the circumjovial planets gravitate towards Jupiter, the circumsaturnal towards Saturn, the circumsolar towards the sun; and, by the forces of their gravity, are drawn off from rectilinear motions and retained in curvilinear orbits." The first corollary is: "There is therefore a power of gravity tending to all the planets. For doubtless Venus, Mercury, and the rest are bodies of the same sort as Jupiter and Saturn. And, since all attraction (by the third law) is mutual, Jupiter will therefore gravitate towards all his own satellites, Saturn towards his, the earth towards the moon, and the sun towards all the primary planets."

On March 28 Newton replied:[5]

"The difficulty you mention, which lies in these words: '*Et cum attractio omnis mutua sit,*' is removed by considering that, as in geometry the word 'hypothesis' is not taken in so large a sense as to include the axioms and postulates, so in experimental philosophy it is not to be taken in so large a sense as to include the first principles or axioms which I call the Laws of Motion. These principles are deduced from phenomena and made general by induction, which is the highest evidence that a proposition can have in this philosophy. And the word 'hypothesis' is here used by me to signify only such a proposition as is not a phenomenon nor deduced from any phenomena, but assumed or supposed without any experimental proof. Now the mutual and mutually equal attraction of bodies is a branch of the third Law of Motion, and how this branch is deduced from phenomena you may see in the end of the corollaries of the Laws of Motion. If a body attracts another body contiguous to it and is not mutually attracted by the other, the attracted body will drive the other before it and both will go away together with an accelerated motion to infinity, as it were by a self-moving principle, contrary to the first Law of Motion, whereas there is no such phenomenon in all nature.

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[5] *Ibid.*, pp. 154-155.

"At the end of the last paragraph but two now ready to be printed off,[6] I desire you to add after the words: 'a God without dominion, providence, and final causes, is nothing else but fate and nature' these words: 'And thus much concerning God; to discourse of whom from the appearances of things does certainly belong to natural philosophy.'

"And, for preventing exceptions against the use of the word 'hypothesis,' I desire you to conclude the next paragraph in this manner: 'For whatever is not deduced from the phenomena, is to be called a hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. And to us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies and of our sea.' "

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[6] This is in the "General Scholium" at the end of the *Principia.*

XV.

In his *Principia,* Newton[1] defined "quantity of matter" as the product of volume and density. He gave no further explanation of the conception of density; but from what he said later on it appears that he supposed the smallest particles of matter to be equally dense and simply put the density proportional to the number of these particles in a given space.[2]

When giving an account of the eleventh section of the first book of the *Principia,* Rosenberger[3] remarked: "Since all the corpuscles of which bodies consist contain equal quantities of matter, and since all attractions are wholly due to matter, all corpuscles exert equal forces outwards. The total force of a body is thus the sum of the forces of the corpuscles, the action of a body outwards must, without exception, be proportional to the number of its corpuscles, and the weight of a body gives a sure measure of its mass. This idea of a body as a system of corpuscles whose unity only becomes evident with bodies that are very distant from one another, forms the transition from the mechanics of points to the mechanics of systems of points or physical bodies. The indication of this transition is, as is always the case with Newton, very slight. 'It is reasonable to suppose,' said he in the scholium at the end of the eleventh section, 'that forces which are directed to bodies should depend upon the nature and quantity of those bodies, as we see they do in magnetical experiments. And when such cases occur, we are to calculate the attractions of the bodies by assigning to each of their particles its proper force, and then collecting the sum of them all.' "

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[1] Book I, Definition I. "It is this quantity that I mean hereafter everywhere under the name of body or mass."

[2] Rosenberger, *op. cit.,* p. 173.

[3] *Ibid.,* p. 192. It must be remembered that the conception of mass and the third law of motion were first used in this eleventh section, which deals with the motions of bodies tending to each other with centripetal forces.

The sixth proposition of the third book is: "That all bodies gravitate towards every planet; and that the weights of bodies towards any planet at equal distances from the center of the planet are proportional to the quantities of matter which they severally contain." To this proposition, the second corollary was: "Universally, all bodies about the earth gravitate towards the earth, and the weights of all bodies at equal distances from the earth's center are as the quantities of matter which they severally contain. This is the quality of all bodies within the reach of our experiments, and therefore, by the third Rule for Philosophizing, is to be affirmed of all bodies whatsoever." To this, Rosenberger[4] added the remark: "For the effect of the whole body is equal to the sum of the effects of its single particles, and therefore, if the particles are all equal, is proportional to the number of the particles or the mass." However, it must be emphasized that Newton nowhere stated explicitly that the mass was proportional to the number of particles. That he thought so, there can, I think, be little doubt.

In the third rule of the *regulae philosophandi* at the beginning of his third book,—the rule, namely, that: "The qualities of bodies, which admit neither intension nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever,"—Newton included among the qualities which can neither be increased nor diminished: extension, hardness, impenetrability, mobility, and inertia, and remarked: "And, because the hardness of the whole arises from the hardness of the parts, we therefore justly infer the hardness of the undivided particles not only of the bodies we feel but of all others.... We conclude the least particles of all bodies to be also all extended, hard, impenetrable, mobile, and endowed with their proper forces of inertia. And this is the foundation of all philosophy. Moreover, that the divided but contiguous particles of bodies may be separated from one another, is matter of observation; and, in the particles that remain undivided, our minds are able to distinguish yet lesser parts, as is mathematically demonstrated. But whether the parts so distinguished, and not yet divided, may, by the powers of nature, be actually divided and separated from one another, we cannot certainly determine. Yet, had we the proof of but one experiment, that any undivided particle, in breaking a hard and solid body, suffered a division, we might, by virtue of this rule, conclude that the undivided as well as the divided particles may be divided and actually separated to infinity."

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[4] *Op. cit.*, p. 214.

The second corollary to the sixth proposition went on:

"If the ether, or any other body, were either altogether devoid of gravity or were to gravitate less in proportion to its quantity of matter, then, because, according[5] to Aristotle, Descartes, and others, there is no difference between that and other bodies but in the mere form of matter, it might, by a successive change from form to form, be changed at last into a body of the same condition as those which gravitate most in proportion to their quantity of matter; and, on the other hand, the heaviest bodies, acquiring the first form of that body, might by degrees quite lose their gravity. And therefore the weights would depend upon the forms of bodies, and might be changed with those forms, contrary to what was proved in the preceding [the first] corollary."

The third corollary will occupy us later, in connection with some of the correspondence between Cotes and Newton. The fourth corollary was: "If all the solid particles of all bodies are of the same density and cannot be rarefied without pores, a void space or vaccum must be granted. By bodies of the same density I mean those whose forces of inertia are in the proportion of their bulks." Now, Rosenberger[6] went rather beyond the facts and remarked: "Newton therefore, in a fourth corollary stated that the solid particles of all bodies are equally dense and that bodies of different density only differ by the magnitude and quantity of their pores."

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[5] This and the five following words were first added in the second edition of the *Principia.*

In the April of 1686, Halley[7] announced the appearance of Newton's *Principia* in an essay which appears to have been inspired by Newton and which, as Rosenberger[8] has remarked, "contains in short indications almost all the foundations of Newton's natural philosophy. Only one principle is still lacking, and that is the idea of molecular gravitation or the composition of the gravitation of bodies out of that of their smallest particles. Everywhere, Halley only speaks of the tendency of the single particles of the body towards a point inside the body—the center of gravitation—, but does not remark that this center is obtained by summation of the effects of all the particles, and that the center of force is only a mathematical fiction. Indeed, he even indicates that the Creator might have ordained this center to be anywhere in bodies, and this—so at least it appears to me—directly contradicts the idea of a molecular gravitation. However, it cannot be decided with certainty whether or no Newton himself, at the time at which Halley drew up his memoir, had formed the idea of molecular attraction and had merely not progressed with it so far that he could communicate it to others; or whether or no Newton had not then grasped the thoughts about molecular forces, and only arrived at this important notion after his communications to Halley. From the *Principia* of 1687, we can conclude that the idea of molecular gravitation together with the conceptions of mass, developed with Newton at a fairly late stage—when, in fact, he was busied with the elaboration of the attraction of extended bodies. Still, it is not very likely that this development was still uncompleted at the time of Halley's announcement."

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[6] *Op. cit.*, p. 215.

[7] Cf. *ibid.*, p. 260, and Rouse Ball, *op. cit.*, p. 62.

[8] *Op. cit.*, p. 263

On this, we may again remark that it is now definitely established that Newton did not discover the theorems on the attraction of spherical bodies until 1685, and apparently did not mention them to Halley until the letter of June 20, 1686, an extract from which is given above. On the other hand, the Newtonian conception of mass, as we have seen from Newton's early and previously unpublished manuscripts, was of much earlier growth. It is probable that Newton made then what seems now to be the obvious step —the way for which was amply prepared by Descartes—of considering the ultimate particles of all bodies to be of the same density, and all the differences of bodies to arise solely from the number of these particles in a given space; and it is not unlikely, as we saw above, that Newton early reached the idea of universal gravitation—of attractions between particles, which vary inversely as the squares of the distances and directly as the masses. If this be so, we must admit that Newton's physical views were, in essentials, completed at an early date, and that the subsequent development of his "philosophy" was preeminently of a mathematical character. Of course, the mathematical work enabled precision and a most attractive appearance of completeness to be given to these physical views. In particular, the purely mathematical determination of the attractions of spherical bodies seems to have played a very important part in this respect, as in others. I cannot, then, agree with Rosenberger as to the comparatively late growth of Newton's chief physical ideas. On another occasion I will trace the fortunes of Newton's views on the ether and on gravitation as an essential property of matter, and will examine Rosenberger's opinions on these subjects. However, it seems to me that Rosenberger has correctly grasped the gist of the Newtonian conception of mass, though his methods of quotation are by no means always unprejudiced. It is important to try to get at Newton's exact meaning, for his notions about mass, ether, and whether or no gravitation is an essential property of matter, were never expressed without caution, which is doubtless commendable, and ambiguity, which is certainly not.

XVI.

Mechanical ideas were applied to celestial events, and not only to terrestrial events before Newton's time. Newton, of course, was the first to carry out this application in a thoroughly scientific manner, but good work in this direction had been done by Descartes, Wallis, Wren, Hooke, Halley, and others. With this extension of ideas came the idea of one and the same body—involving therefore something constant—having different weights according to its positions in space.[1] Mach referred with approval to some of Rosenberger's[2] remarks on the Newtonian conception of mass. "It should be observed," said Mach,[3] "that the notion of mass as quantity of matter was psychologically a very natural conception for Newton, with his peculiar development. Critical inquiries as to the origin of the concept of matter could not possibly be expected of a scientist in Newton's day. The concept developed quite instinctively; it is discovered as a datum perfectly complete, and is adopted with absolute ingenuousness. The same is the case with the concept of force. But force appears conjoined with matter. And, inasmuch as Newton invested all material particles with precisely identical gravitational forces, inasmuch as he regarded the forces exerted by the heavenly bodies on one another as the sum of the forces of the individual particles composing them, naturally these forces appear to be inseparably conjoined with the quantity of matter."

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[1] Cf. Mach, Mechanics, p. 192. Cf. also pp. 194-195, 216-222, 536-537, 558-560 for a criticism of the Newtonian conception of mass.

[2] *Op. cit.*, p. 192. [3] *Op. cit.*, p. 536.

On the enumeration of the ultimate particles to get the measure of mass, Mach[4] remarked: "We do not find the expression 'quantity of matter' adapted to explain and elucidate the concept of mass, since that expression itself is not possessed of the requisite clearness: And this is so, though we go back, as many authors have done, to an enumeration of the hypothetical atoms. We only complicate, in so doing, indefensible conceptions. If we place together a number of equal, chemically homogeneous bodies, we can, it may be granted, connect some clear idea with 'quantity of matter,' and we perceive also that the resistance the bodies offer to motion increases with this quantity. But the moment we suppose chemical heterogeneity, the assumption that there is still something that is measurable by the same standard, which something we call quantity of matter, may be suggested by mechanical experiences, but is an assumption nevertheless that needs to be justified." It must be remarked that Newton himself appears to be one of those authors referred to by Mach;[5] and that the Newtonian conception, involving as it does the hypothesis of the sameness in constitution of the ultimate particles of bodies which are chemically heterogeneous, is perfectly clear: the only defect is that it rests on mere hypothesis, which can be neither proved nor disproved. Indeed, it seems that Newton's boast—or expression of caution—*hypotheses non fingo* only applied to such hypotheses as he consciously and explicitly made.

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[4] *Ibid.*, pp. 216-217.

[5] Heinrich Hertz, in his well-known work of 1894 on the principles of mechanics, also defined mass as the number of particles.